matter how many degrees of freedom the resonator possesses, or what the form of its potential energy. Indeed, according to this argument, equation (2) is proved for any dynamical system, *e.g.* the molecules of a gas.

It is, however, known that equation (2), with Planck's meaning of k, is true if, and only if, the energy of each dynamical system is expressible as the sum of two squares. It can, indeed, be shown directly that this latter condition is exactly the condition that Prof. Planck's assumed basis of probability calculations shall be a legitimate basis, *i.e.* shall be independent of the time. Happily, this condition of the energy being a sum of two squares may be supposed to be satisfied by Planck's resonators, so that we may regard equation (1) as true for such resonators. The equation has, however, no physical meaning, owing to the presence of the arbitrary small quantity ϵ, and can acquire a physical meaning only by putting $\epsilon=0$. It then leads merely to equation (2), which can be obtained much more readily from the theorem of equipartition.

Taking $\mathbf{u}d\nu$ to be the law of radiation, where ν is the reciprocal of the period of vibration, Planck introduces from his first paper the equation

$$\mathbf{u}=(8\pi\nu^2/c^3)\mathrm{U} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which in combination with equation (2) would lead to the law of radiation,

$$(8\pi k/c^3)\mathrm{T}\nu^2 d\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and this, on replacing ν by c/λ, becomes

$$8\pi k\mathrm{T}\lambda^{-4}d\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which agrees with my own result. Planck arrives at equation (3) by the help of his assumption of "nätürliche Strahlung," but I believe it will be found that this "assumption" is capable of immediate proof by the methods of statistical mechanics. Except for this, and the other differences already stated, the way in which expression (5) has been reached in the present letter is identical, as regards underlying physical conceptions, with the way in which it has been obtained by Lord Rayleigh and myself.

Planck does not reach expression (5) at all, as he does not pass from equation (1) to equation (2). Instead of putting $\epsilon=0$, he puts $\epsilon=h\nu$, where h is a constant, and this leads at once to his well known law of radiation. It will now be clear why Planck's formula reduces to my own when $\lambda=\infty$. For taking $\lambda=\infty$ is the same thing as taking $\nu=0$, or $\epsilon=0$.

The relation $\epsilon=h\nu$ is assumed by Planck in order that the law ultimately obtained may satisfy Wien's "displacement law," *i.e.* may be of the form

$$\nu^3/c^3 f(\mathrm{T}/\nu)d\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

This law is obtained by Wien from thermodynamical considerations on the supposition that the energy of the ether is in statistical equilibrium with that of matter at a uniform temperature. The method of statistical mechanics, however, enables us to go further and determine the form of the function $f(\mathrm{T}/\nu)$; it is found to be $8\pi k(\mathrm{T}/\nu)$, so that Wien's law (6) reduces to the law given by expression (4). In other words, Wien's law directs us to take $\epsilon=h\nu$, but leaves h indeterminate, whereas statistical mechanics gives us the further information that the true value of h is $h=0$. Indeed, this is sufficiently obvious from general principles. The only way of eliminating the arbitrary quantity ϵ is by taking $\epsilon=0$, and this is the same as $h=0$.

Thus it comes about that in Planck's final law

$$\frac{8\pi ch}{\lambda^5}\frac{1}{e^{ch/k\lambda\mathrm{T}}-1}d\lambda \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

the value of h is left indeterminate; on putting $h=0$, the value assigned to it by statistical mechanics, we arrive at once at the law (5).

The similarities and differences of Planck's method and my own may perhaps be best summed up by saying that the methods of both are in effect the methods of statistical mechanics and of the theorem of equipartition of energy, but that I carry the method further than Planck, since Planck stops short of the step of putting $h=0$. I venture to express the opinion that it is not legitimate to stop short at this point, as the hypotheses upon which Planck has worked lead to the relation $h=0$ as a necessary consequence.

Of course, I am aware that Planck's law is in good agreement with experiment if h is given a value different from zero, while my own law, obtained by putting $h=0$, cannot possibly agree with experiment. This does not alter my belief that the value $h=0$ is the only value which it is possible to take, my view being that the supposition that the energy of the ether is in equilibrium with that of matter is utterly erroneous in the case of ether vibrations of short wave-length under experimental conditions.

J. H. JEANS.

On the Spontaneous Action of Radium on Gelatin Media.

SINCE my communication to NATURE on the subject of the experiments in which I have been for some time past engaged, my attention has been directed to the fact that M. B. Dubois, in a speech at Lyons last November, stated that he had obtained some microscopic bodies by the action of radium salts on gelatin bouillon which had been rendered "aseptic," but in what manner it is not stated.

I write to direct attention to the fact, as also to add that M. Dubois's experiments were quite unknown to me.

Moreover, the theory that some elementary form of life, far simpler than any hitherto observed, might exist and perhaps be brought about artificially by "molecular and atomic groupings and the groupings of electrons"—in virtue of some inherent property of the atoms of such substances as radium—was pointed out in my article on the "Radio-activity of Matter" in the *Monthly Review*, November, 1903, whilst the experiments which I have been carrying out to verify this view have been for a long time known in Cambridge.

Although I did not make a speech on the subject, I demonstrated the growths to many people at the Cavendish and Pathological laboratories early in the Michaelmas Term last year.

So momentous a result as it seemed required careful confirmation, and much delay was also caused in taking the opinions of various men of science before I ventured to write to you upon the subject.

That M. Dubois's experiments have been made quite independently I do not entertain the slightest doubt.

Some critics have suggested that these forms I have observed may be identified with the curious bodies obtained by Quincke, Lehmann, Schenck, Leduc and others in recent times, and by Rainey and Crosse more than half a century ago; but I do not think, at least so far as I can at present judge, that there is sufficient reason for so classifying them together. They seem to me to have little in common except, perhaps, the scale of being to which as microscopic forms they happen to belong.

JOHN BUTLER BURKE.

The Problem of the Random Walk.

CAN any of your readers refer me to a work wherein I should find a solution of the following problem, or failing the knowledge of any existing solution provide me with an original one? I should be extremely grateful for aid in the matter.

A man starts from a point O and walks l yards in a straight line; he then turns through any angle whatever and walks another l yards in a second straight line. He repeats this process n times. I require the probability that after these n stretches he is at a distance between r and $r+\delta r$ from his starting point, O.

The problem is one of considerable interest, but I have only succeeded in obtaining an integrated solution for *two* stretches. I think, however, that a solution ought to be found, if only in the form of a series in powers of $1/n$, when n is large.

KARL PEARSON.

The Gables, East Ilsley, Berks.

British Archæology and Philistinism.

AT the end of the second week in July two contracted skeletons were found in a nurseryman's grounds near the famous British camp at Leagrave, Luton. Both were greatly contracted; one, on its right side, had both arms straight down, one under the body the other above; the other skeleton lay upon its left side, with the left hand

under the face and the right arm straight down. Both were probably female, and upon the breast of one was a fine bronze pin seven inches long with three pendant ornaments, and three discs of bronze, one plated with gold. Other bronzes of great interest were found with the second skeleton.

I do not write to describe the bones and ornaments, but to make public the conduct of the Luton authority. A most intelligent workman lives close to the site of the discovery—one Thomas Cumberland—a man who has studied the antiquities of the district for many years, and to whom antiquaries are indebted for great and freely given assistance. This man was on the spot at once, and clearly and correctly stated the age of the bones and ornaments as British or late Celtic. Notwithstanding this information, the local police insisted on an inquest, although the bones were broken to pieces and in the highest degree friable. I went to the nursery and confirmed Mr. Cumberland's determination, made drawings of the bronzes, and such an examination of the bones as circumstances would permit. The coroner refused to hold an inquest, and so had no authority to make any order, but he wrote and "suggested" that the bones should be buried in the parish churchyard. Armed with this "suggestion," the relieving officer ordered an undertaker to carry off the bones, which he did, in spite of the protest of the nurseryman, who informed him that they had been given to me and were my property. He was ordered to put the bones in coffins and bury them in the churchyard of Biscot. The undertaker took the bones to his shop at Luton. I at once applied to the relieving officer for permission to examine and measure some of the bones. I clearly explained to him the nature and importance of the discovery, and the trifling nature of the favour asked. This official replied in a curt and rude manner, and simply said, "I have no authority; you must apply to the coroner."

I repeatedly wrote to the undertaker to delay the funeral for a few days. I twice wrote to the coroner in an urgent but most respectful manner, and pointed out the importance of the discovery, which, indeed, is quite unique in this district, but all to no purpose. He said he had not given the "order" for burial, and he refused to interfere, but he wrote to the undertaker and said, "I can give no consent or authority in any way, but must leave you to carry out the arrangement which has been come to with you." I wrote letters for six days to the different persons concerned, but to no effect; they would have a funeral, and the police now actually demanded the bronzes from the owner. The property is freehold.

Well, on Wednesday last the two coffins were screwed up at Luton and taken in a hearse to Biscot churchyard, where the vicar, in the presence of a policeman, officiated. Shining breastplates were screwed on to the coffins inscribed, "Bones found at Leagrave, July 1905." Amongst the bones in the coffins were several non-human examples, a rib bone of a sheep, a piece of a rib of beef, a bone of a rabbit, and another of roebuck.

Dunstable. WORTHINGTON G. SMITH.

Graphical Solution of Cubic and Quartic Equations.

SOME years ago you published some interesting communications in regard to the graphical solution of cubic and quartic equations (vol. lxi. p. 55, vol. lxiii. pp. 515 and 609, vol. lxiv. p. 5). The solutions then given give only the real roots of the equation. I therefore take the liberty of directing attention to the following method, which gives the roots of cubic and quartic equations whether the roots are real or complex, and may be applied to equations of higher degree, with more complicated results.

A cubic equation with real coefficients may be reduced by a simple real transformation to the form

$$z^3+qz+1=0,$$

where q is real, and since the sum of the roots of this equation is zero, they may be written in the form

$$\begin{gathered}-2y\\ y+\sqrt{x}\\ y-\sqrt{x}.\end{gathered}$$

If, now, we form the symmetric functions, we have

$$\begin{gathered}3y^2+x=-q\\ 2(y^3-xy)=1.\end{gathered}$$

Hence if we draw the fixed curve

$$y^3-xy-\tfrac{1}{2}=0,$$

the coordinates of the points where it is cut by the movable parabola

$$3y^2+x+q=0$$

give the roots of the equation

$$z^3+qz+1=0,$$

i.e. if x_0 and y_0 are the coordinates of any such point, $-2y_0$ and $y_0\pm\sqrt{x_0}$ are the roots of the given equation.

In like manner a quartic equation with real coefficients may be put into the form

$$z^4+qz^2+z+s=0,$$

where q and s are real, and its roots may be put into the form

$$\begin{gathered}v\pm\sqrt{w_1}\\ -v\pm\sqrt{w_2}\end{gathered}$$

and, forming the symmetric functions, we have

$$2v^2+w_1+w_2=-q \quad (1)$$
$$2v(w_1-w_2)=-1 \quad (2)$$
$$v^4-(w_1+w_2)v^2+w_1w_2=s \quad (3)$$

and if we put

$$\begin{gathered}4v^2=y\\ (w_1-w_2)^2=x\end{gathered}$$

we find by simple elimination

$$xy=1,\ (y+q)^2-(x+4s)=0.$$

Hence the intersections of the fixed hyperbola with the movable parabola give values for x and y from which v, w_1, and w_2 may be calculated.

If we eliminate x from the two equations last written, we have

$$y^3+2qy^2+q^2y-4sy-1=0.$$

Hence there is always at least one positive value for y, therefore a real value of v; also, since $xy=1$, a positive value for x, therefore a real value for w_1-w_2; and since from (1) w_1+w_2 is real, real values for w_1 and w_2.

H. IVAH THOMSEN.

1928 Mt. Royal Terrace, Baltimore, Md., June 7.

THE PRESENT POSITION OF THE CANCER PROBLEM.

THE term "cancer" is in common parlance indiscriminately applied to all tumours the growth of which is unlimited and generally rapid, which tend to recur after removal by operation, and particularly which reproduce their like (the secondary or metastatic growths) in parts of the body remote from the original seat of disease. Pathologically there are various forms of "cancer," or malignant disease, but there is no need to deal with these here, and it may be stated that there is no sharp line of demarcation between the so-called benign and the malignant growths; there is a series of connecting links between the two. Malignant disease is an important cause of death. According to the last published report of the Registrar General (1903), the death-rate from this cause per 1000 living was 0·87; for comparison that for pulmonary tuberculosis (consumption) may be quoted; this was 1·2.

Moreover, it is a common belief that cancer is on the increase; people remark how much more frequently it is heard of now than formerly, and apparently the statistics support this view, for the cancer death-rate, which was 0·56 in 1884, has steadily increased, and is now 0·87, as stated above. Competent statisticians, however, doubt whether the increase is real or only apparent, and partly due to more accurate diagnosis and to a greater tendency to seek medical advice. During the last two or three decades surgery has made